CHANGES IN THE BODILY FORM OF DESCENDANTS OF IMMIGRANTS

By FRANZ BOAS

UNDER this title I have published the results of my investigations on the anthropometry of immigrants and their descendants, undertaken for the United States Immigration Commission. A partial report was asked for by the Commission and submitted to Congress on December 16, 1909, and published about March, 1910. It was stated in the report (p. 6) that the investigation was not complete. An abstract of the complete report was submitted to Congress on December 3, 1910, and issued on March 17, 1911. The final report was presented on December 5, 1910, by the Secretary of the Commission, submitted to Congress on June 8, 1911, printed in September, 1911, and issued in May, 1912. It was reprinted and published by the Columbia University Press in New York in 1912.

I may summarize the principal results of this investigation as follows:

1. American-born descendants of immigrants differ in type from their foreign-born parents. The changes which occur among various European types are not all in the same direction. They develop in early childhood and persist throughout life (Partial Report, pp. 7–16; Abstract, pp. 11–28; Final Report, pp. 55–56, and tables, pp. 10–55).

2. The influence of American environment makes itself felt with increasing intensity, according to the time elapsed between the arrival of the mother and the birth of the child (Partial Report, pp. 17–22; Abstract, pp. 29–37; Final Report, pp. 57–64, 99–115).

3. The observations on intraracial heredity show an increased variability of children of dissimilar parents, which proves a regression of the children to either parental type, not a regression to the mid-parental type (Abstract, pp. 54–55; Final Report, pp. 76–78, 153–154).

4. The head measurements show the same acceleration of growth during the prepubertal period as has long been known for measurements of the bulk of the body, i. e., stature and weight (Abstract, pp. 55–57; Final Report, pp. 78–79, 137–151).

5. The average stature of children decreases with the size of the family (Partial Report, p. 28; Abstract, p. 57; Final Report, pp. 79–80, 161–166).

Incidentally a number of problems were touched upon which are, however, of secondary importance in relation to the whole problem, and the investigation of which was necessary for the correct interpretation of the observations referred to before.

The comparison of immigrants and their descendants necessarily refers to groups which immigrated at different periods. For instance, 15-year old American-born boys are children of parents who immigrated more than 15 years ago; while 15-year old foreign-born boys are children of parents who immigrated less than 15 years ago. If, therefore, the constitution of the immigration representing a certain people changed, there would be an apparent change of type, which in reality would reflect only the differences in type of the immigrants of various periods. The investigation in question showed the following:

6. Individuals who immigrated in any particular year and the descendants of mothers who immigrated in that year show the same differences in type as have been observed for the whole series (Abstract, pp. 44–46; Final Report, pp. 65–69, 108, 111, 113).

7. Foreign-born parents when compared with their own foreign-born and American-born children exhibit the same types of differences for the children as the whole series (Partial Report, pp. 44–50; Abstract, p. 47; Final Report, pp. 69–70, 117–128).

8. When the Hebrew boys are classified according to their pubescence in groups of about equal physiological development, the same differences persist (Partial Report, pp. 25–28; Abstract, pp. 38–43; Final Report, pp. 126 et seq.).

9. It seems that after the panic of 1893 a decrease in the general development of the Hebrew immigrants appeared, which continued for several years (Partial Report, pp. 28–29; Final Report, p. 65).

											AGES											
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
Bohemians, Poles, Hungarians, Slovacs — males	15.4	16.0	14.9	15.3	14.4	13.7	13.9	13.7	13.2	12.3	12.6	11.7	12.0	11.1	12.6				10.2			
females	15.8	15.2	14.9	15.5	14.6	14.2	13.8	13.5	13.5	12.9	12.0	13.1	12.8					11.0				
Hebrews — males	12.5	13.5	11.7	12.1	10.6	10.1	10.5	10.1	10.0	9.6	9.3	9.3	9.7	9.0	8.4				7.5			
females	12.6	12.0	11.8	12.1	11.4	10.7	11.4	10.7	11.3	10.1	10.4	9.6	10.1					9.0				
Sicilians — males	10.2	10.9	10.0	10.4	8.4	9.4	9.0	8.5	7.9	8.5	7.8	8.6	6.3	6.0	7.5	8.0	7.6	5.9	5.6	6.3	6.1	5.6
females	10.6	10.7	10.3	10.4	10.5	9.4	8.9	9.2	8.3	7.4	6.9	8.3	7.7	7.5	6.4	6.5	6.9	6.9	5.9	6.5	6.8	6.0
Neapolitans — males	11.3	10.6	11.3	10.7	10.3	9.6	8.8	9.0	8.3	8.2	8.1	8.0	7.7	8.5	6.5	6.4	7.9	7.2	8.2	6.2	4.0	5.6
females	11.1	11.3	11.3	11.1	10.6	9.9	10.1	7.9	9.0	8.4	8.8	7.5	7.8	8.3	8.1	7.2	5.9	7.9	8.5	7.7	8.9	6.5

10. I think the investigation on color of hair also deserves special mention. Basing my inquiry on the assumption that the variations of hair color in any particular people follow the exponential law, I have shown that numerical values for pigmentation can be obtained (Final Report, pp. 93–98). I have divided the whole series of pigmentation from black to ash-blond in 20 equidistant steps, 0 being black, 20 ash-blond, but not without pigment like the hair of albinos. In this manner the results given in the accompanying table showing the degree of darkening with increasing age were obtained.

According to this table in the male the darkening amounts to nearly 5 units—one-fourth of the whole scale of colors. If the amount of darkening of females in the first two groups is less, we have to allow for the dyeing of hair, which is practised by many women, and also for the use of false hair by married Jewesses. For this reason I do not lay great stress upon the figures obtained from observations on adult f e m a l e s, except among the Italians. It would seem as though among them the hair of women averages a little lighter than that of men. This apparent difference may, however, be due to the lighter color of the tips of the long hair of women. The process of darkening progresses at least until the twenty-

sixth year, if not longer. An attempt to calculate the annual amount of darkening for the Hebrews shows this very clearly. For dark-haired as well as for light-haired groups the darkening amounts to about 0.2 points a year.

My conclusions have been assailed by a number of critics. The questions here summarized seem of sufficient importance to justify a reply to the various objections raised.

I must apologize to anthropologists familiar with the methods of anthropometry for the space taken by a discussion of the criticisms by Mr Radosavljevich, which appeared in the July–September, 1911, number of this journal, pages 394–436 (issued in February, 1912), in which the author assumes the pose of an expert, with what right will appear from the following remarks. Since the *American Anthropologist* submits contributions before acceptance to the judgment of authorities, and since, nevertheless, the article has found its way into the journal, it would seem that a discussion of certain elementary facts of anthropometrical method may be useful not alone to the reader unfamiliar with the subject.

Before entering into the various criticisms, I have to make a few general remarks on Mr Radosavljevich's paper. Among other recriminations he accuses me of inaccuracy of calculations (l. c., p. 412). It is unfortunate for his argument that the disagreements in calculations are solely due to the faulty method that Mr Radosavljevich has applied in taking his arithmetical means. He gives each value equal weight, while, as is taught in elementary arithmetic, each should be given a weight according to the number of cases of its occurrence. The second discrepancy that he has found (p. 422) is due to his faulty arithmetic.

In sharp contrast to his criticisms based on my failure to quote literature,—which I deem unnecessary when the works quoted do not serve a specific purpose germane to the work in hand,[1]—is his

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[1] On page 427 he accuses me of misuse of literary evidence. He enumerates without critique the opinions of authors who have claimed that the cephalic index during growth decreases, or that it is stationary, or that it increases. He objects then to my statement that it decreases. The proof is given in the Abstract, pp. 55–57, which Mr Radosavljevich never mentions, and more fully in the Final Report, pp. 137–151. The conclusion that because I do not quote certain literature, I do not know it,

own failure to take notice of the abstract of my Final Report which appeared eleven months before the publication of his review, and the misleading inaccuracy of his quotations. Thus, on page 425, he reproduces from page 11 of my Partial Report, three headforms, and says (p. 427): "These sketches of headforms are, therefore, based not on his [Boas's] measurements but on pure imagination," suppressing my note which is found in the text and in the legend of the sketches in question: "These sketches are intended only to give an impression of the change in proportions. They do not represent the headform in detail."

Again on page 423 he alleges he has found "a few more" errors in tabulations, but repeats, as new, statements made previously by him on page 417. (In regard to these tabulations see p. 540.)

On page 421 he says: "We are surprised indeed that he has not a single table in his appendix referring to Sicilians, yet throughout his report he comes again and again to his discovery that very short-headed Jews are becoming 'long-headed' and very 'long-headed' Sicilians 'short-headed.'" He mentions this matter again on page 410. Still I say expressly in the Report which he reviews (p. 30) that "the present report is based only on a partial discussion of the Hebrew material and the generalized averages of the Sicilians and Calabrians."

He continues on page 421: "Instead of these [tables relating to Sicilians] he gives tables occupying eleven full pages, relating to his old study of the stature of Toronto children, and having no connection at all with his study of the descendants of immigrants." He does not say that the tables were to elucidate an entirely new point, the dependence of stature upon size of family (Partial Report, p. 28), which has a direct relation to the problem in hand and for the elucidation of which the tables are necessary.

Another complete change in my meaning is brought about through omissions in the following quotation from page 39 of my

shows a curious lack of logic. In the Abstract, pp. 55–56 (also Final Report, pp. 144 et seq.), Mr Radosavljevich might have found utilized the literature which he quotes. I refer him also to my discussions of the subject in *Science*, n, s., IV, 1896. pp. 50–51.

Report (1. c., p. 394). Words printed in italics have been omitted by Mr Radosavljevich.

" *The changes in the amounts of measurements for different periods are, however, so irregular, that even with the numbers thus obtained no very clear results appear.* It would seem that on the whole there has been a decrease in the length of the head, width of the head, and width of the face since the middle of the last century, *but the change that has taken place is rather slight.* A feature that is particularly noticeable is the general drop of all the absolute measurements after the year 1894. An attempt to combine all the material, adult and children, for these years, brings out the sudden drop after 1893 even more clearly; and a similar phenomenon is repeated between the years 1907 and 1909. For this reason I am inclined to believe that the type of immigrants is directly affected by financial panics. *This can be due only to a selection that takes place in Europe, and which may also be affected by the return emigration from the United States to Europe. The material, so far as it has been discussed, does not give a definite answer to this important question, the solution of which would require a series of parallel measurements taken in Europe.*"

Throughout his article he assumes that this statement, which is only an incident in my work, is the main thesis of my report.

I. METHOD OF MEASUREMENT

So far as I am aware the only objections raised against the method of measurement are those made by Mr Radosavljevich in the article quoted (pp. 419, 420, 423). He objects to lack of sharp definition of age, length of head, width of head, width of face, stature, and weight, data and measurements which are at present taken on the living according to uniform methods,—namely, age according to the usage of European languages as age at last birthday, the head and face measurements as maximum measurements, length of head from the glabella, stature and weight in stocking feet and without clothing,—unless different methods are specifically stated, as on pages 65 and 82 of my Final Report. Nobody misunderstands these methods, of which numerous examples may be found in anthropological literature. As a matter of fact Mr Radosavljevich

himself understands them, too, for on page 420 he speaks of length and breadth of head without any specification, assuming that the reader knows what he means.

Similarly his objection to lack of definition of physiological development (p. 423) is groundless. The statement (Partial Report, p. 34), "At the same time observations were made on pubescence as a means of determining the approximate physiological development. The method of these observations has been developed by Dr C. Ward Crampton," should make it clear to anyone that Dr Crampton's methods were followed. The description of this method is found in Dr Crampton's publications. Mr Radosavljevich might have read on page 33 that Dr Crampton himself organized the work in the schools.

More serious is the question of the accuracy of the observations. He states that "Bertillon, Martin, and other anthropometricians" (myself included) "require from their students an exactness for the length and breadth of the head within the limits of about 1 mm. If the difference is as much as 2 it is regarded as a discrepancy beyond which measurements cease to make identification of the subject measured possible, and if it is over 2, mistakes of a serious character are made beyond which non-identity can be made. The personal equation of Boas' thirteen observers who made successive measurements by way of preliminary practice on each other is in every observer above 0.5 mm." (p. 420). The last statement is incorrect. In reality the distribution of personal equations of the observers in regard to those head and face measurements was as follows (Final Report, p. 92):

0–0.4 mm.	31	times	2.0–2.4 mm.	1	time
0.5–0.9 "	29	"	2.4–2.9 "	–	"
1.0–1.4 "	12	"	3.0–3.4 "	1	"
1.5–1.9 "	3	"			

Among these the two highest groups are represented by very few individuals only (17 for 2.2, 38 for 3.3), and the personal equations have therefore a very small weight.

Mr Radosavljevich distinguishes neither between error of

observation and personal equation nor between the effect of errors of observation upon an anthropometrical series and the effect of such errors upon measurements made for the purpose of identifying individuals.   The personal equation is due to peculiarities in the method of measuring—like habitual differences in the choice of terminal points of the lines measured, or greater or lesser pressure. We try to eliminate these by practice, although they can never be entirely overcome.   The error of observation is due to accidental errors affecting each measurement.   While in measurements taken for the purpose of identification all errors *must* be restricted to a very low limit, equal accuracy is not *absolutely necessary* in measurements intended as a description of a series.   We shall see later that most series can be adequately described by two values: the average and the mean square variability.   In all these cases errors are admissible that do not influence either of these values beyond the limits of their accuracy.   The mean square variability is determined as the square root of the average of all the squared deviations from the average; or, if the average is called a, the difference between any observation and the average x, the variability σ, the number of observations n, $\sigma = \pm \sqrt{\Sigma x^2/n}$, where Σx^2 indicates the sum of all the values of x^2.   Then the mean square error of the average may be expressed by $\epsilon = \pm \sigma/\sqrt{n}$.

According to the theory of probabilities this means that we may bet as follows, that the true average of an infinitely long series lies between

$$a - \epsilon \quad \text{and} \quad a + \epsilon \qquad 68 \quad \text{against} \quad 32$$
$$a - 2\epsilon \quad `` \quad a + 2\epsilon \qquad 95 \quad `` \quad 5$$
$$a - 3\epsilon \quad `` \quad a + 3\epsilon \qquad 99.5 \quad `` \quad 0.5$$

The accuracy of σ^2 is measured in the same way by the error of $\pm \sigma^2 \sqrt{2/n}$.

In a sufficiently long series an accidental error of observation has no effect upon the average.   For the mean square variability σ_1 of the series which is affected by an accidental error of observation whose mean square is δ we have

$$\sigma_1^2 = \sigma^2 + \delta^2$$

The value of σ_1^2 will remain within the limits of its mean square error as long as

$$\delta^2 < \sigma^2 \sqrt{2/n}$$
$$\delta < \sigma \sqrt[4]{2/n}$$

Thus we find the following admissible maxima for δ:

For $n = 100$ $\delta < 0.378\sigma$

        $n = 200$ $\delta < 0.316\sigma$

        $n = 800$ $\delta < 0.224\sigma$

For the length of head σ is about 6 mm. Therefore, the mean square error of observation which will still leave the variability within the limits of its mean square error is, for a series of

100 observations	2.4 mm.
200 "	1.9 "
800 "	1.3 "

Furthermore, a certain maximum value can be shown to exist for the exactness of anthropometric measurements. The process of rounding off to the nearest millimeter results in a mean square error of the variability of nearly ± 0.3 mm., provided the actual measures are evenly distributed over the fractions of millimeters. Taking further into consideration the varying consistency and the consequent unequal yielding to pressure of the soft parts covering the skull and zygomatic arches, and the irregularities of the form of the occiput with its sharp or irregular ridges, an exactness of more than ± 0.5 mm. is practically impossible to obtain. In individuals whose true measure lies on a half millimeter it will always be more, owing to the process of rounding off.

Considering the effect of rounding off, the following approximate mean square errors of the length of head result for the various observers, calculated from the table on page 85 of the Final Report:

Le.	± 0.4 mm.	M. F.	± 0.5 mm.
Br.	± 0.5 "	Ja.	± 0.5 "
O. S.	± 0.5 "	T. M.	± 0.5 "
Rp.	± 0.5 "	H. L.	± 0.5 "

Be.   ± 0.7 mm.   L. W.   ± 0.8 mm.

L. F.   ± 0.7 "   Lu.   ± 0.8 "

A. G.   ± 0.8 "

It thus appears that the accuracy is nearly the maximum attainable, and certainly entirely within the limits of the accuracy of the variability.

When it is the object of anthropometric studies to compare the variabilities of different series, this may generally be done without hesitation, so long as the increases of all the variabilities due to the accidental error of observation are the same, particularly since their accuracies are only slightly affected. A case of this kind occurs in our study in the discussion of heredity (pp. 153 et seq.).

The personal equations of the observers, given on page 92, are also without influence upon our results, since in the foreign-born and American-born of the same series care was taken to have the same observers represented with approximately the same relative frequency of observations.

Mr Radosavljevich also criticizes me (p. 415) for not measuring the height of the head, on account of the gratuitous assumption that it might have yielded more important results than other measurements. My reason is clear and simple: the measurement of the distance between vertex and ear, which has to be measured as a projection on the median plane of the body, is so inaccurate that I do not use it on the living. In this I am in the good company of other experienced anthropologists, like Professor von Luschan.

On page 424 he reproaches me for using a scale of colors of hair that I arranged myself, instead of using the colors of Fischer, which proved impracticable on account of their greenish tinges. In my Final Report a special chapter is devoted to this subject (pp. 93–98), in which I explain that my aim was a numerical determination of pigmentation, and a numerical determination of the increase of pigmentation with age, a study which had not been made before.

He also thinks there is no scientific reason for not using Martin's eye colors. Since these are too far apart to be used for numerical determination of pigmentation, and the classification results in

very strong personal equations in making estimates of colors, I preferred to omit eye colors from the study. For skin colors exposure to air and light causes such large variations in the individual that these also were excluded.

At the same place he objects to my making copies of the valuable anthropometric records that have been kept in the Newark Academy, without describing the method of measurement in detail. He claims that I give the blank on page 34. It has not been reproduced in my report, neither on page 34 nor elsewhere. He repeats correctly that I intended to use these records for the sake of studying American families settled in this country for several generations and living under more favorable conditions. Since these have not been treated in any of the tables of my reports, there is no cause for criticism just yet.

2. STATISTICAL PRESENTATION OF RESULTS

I have already pointed out that the errors in calculation which Mr Radosavljevich believes to have found are miscalculations on his part. He also calls attention (pp. 416, 417, and repeated as something new on p. 423) to discrepancies in the numbers treated in Table IV, pp. 17–19 of my Partial Report, which refers to the measurements of length and width of head, width of face, and cephalic index of groups of Hebrews born more or less than ten years after the mother's immigration. This discrepancy is due to the fact that, after the classification for the cephalic index had been made, the cards were arranged for other purposes. Later, when I decided to calculate the values for the absolute measurements, the same classification of the material was made once more. Owing to the necessarily arbitrary classification of those born just ten years after the mother's immigration (see Abstract, p. 34; Final Report, pp. 110, 111), it was impossible to repeat exactly the same classification. Since the validity of a statistical result does not depend upon the fact that the series compared contain the same individuals, this does not affect the study, and I did not feel particularly called upon to explain the differences in numbers which are obvious, since they are all contained in the same table. Since

I adopted later on a more satisfactory method of determining the influence of time elapsed since the immigration of the mothers (Abstract, pp. 44–46; Final Report, pp. 67–69, 108, 111, 113, 115), a recasting of these tables for the final report seemed particularly unnecessary.

An error in the count of 9-year-old American-born Hebrew boys has been pointed out by me on page 102 of the Final Report.

The other discrepancies to which Mr Radosavljevich refers on page 422 are the following:

	CEPHALIC INDEX	LENGTH OF HEAD	WIDTH OF HEAD
Table II, 1900–04, pp. 58–60	214 cases	213 cases	213 cases
Table II, 1890–94, pp. 62–64	162 "	161 "	163 "
Table III, 20 yrs. and over, pp. 68–72	764 "	763 "	763 "
Table IV, 14 yrs., pp. 78–82	24 "	24 "	23 "
Table IV, 18 yrs., pp. 78–82	65 "	66 "	67 "

In every one of these cases one observation has been thrown out for some valid reason, and it did not seem necessary to change the others because the result was not appreciably affected.

Mr Radosavljevich's difficulties in regard to the numbers measured (p. 421) would have disappeared if he had consulted the "Abstract," in which all the numbers of calculated observations are contained.

The numbers of Hebrews in the various series do not agree and can not agree (p. 421), because not all the necessary information for the varied statistical classifications can be obtained in every case, and cases without the necessary information naturally drop out of the particular series.

But to come to more important points. Mr Radosavljevich thinks that my considerations are based on "mere averages,—a method which has been condemned both in America and Europe," and, I may add, a condemnation to which I have added my liberal share. This question is intimately connected with the definition of what constitutes a biological type, and I must therefore say a few words on this trite topic.

Since all biological phenomena are variable phenomena, the biological type, i. e., all the individuals constituting a group, must be described by an enumeration of the frequencies of occurrence of all the variates constituting the type under discussion.   The fact that anthropologists are in the habit of calling heads of a length-breadth index of 80 and more, brachycephalic heads, does not constitute brachycephaly a distinct biological type, but is a mere convenience of description.   In the same way it is merely a convenience of description if we call a people a brachycephalic people in which the arithmetical mean of the head index falls in the group of brachycephaly, and in which also the majority of individuals are brachycephalic.   The terms dolicho-, meso-, and brachy-cephaly have only a meaning as descriptive terms, not as biological types. Owing to this frequent misunderstanding and the erroneous opinion that these groups have really been proved to be distinct biological races, I have avoided for years these terms, notwithstanding their convenience, and speak only of more or less rounded, respectively, elongated heads.   Mr Radosavljevich thinks that all the brachycephalic individuals in a certain people form a biological type, because the same conventional term is used to describe them, while in reality they are only a part of the whole series of variates of a type.

It will be understood that my remarks do not signify that there is no meaning in the distinct headforms which Retzius recognized with great acumen, thus laying the foundation of modern anthropology.   The essential points are: that the arbitrary limits of the indices of 75 and 80 were invented only to classify conveniently the heterogeneous material; that the distribution of head indices in a people is a most important means of describing its characteristics; and that the fact that some individuals have an index lower than 80 and others higher than 80 does not prove that we are dealing with a mixed type.   I shall speak of the question of mixed types later on, when discussing Professor Sergi's views (pp. 558 et seq.).

Bearing in mind the definition of a type, which is purely descriptive and contains no theory in regard to its homogeneous or multiple origin, we may apply what has been said above, that the

exact description of a type requires the statement of the exact relative frequency of occurrence of every form that belongs to the type; or, in case of a measurement, the accurate statement of the relative frequency of every measurement.

With the short series at our disposal this exact description is impossible to attain, for a determination of the exact distribution of frequencies would require for each series numbers of cases so large that ordinarily they do not exist in nature. The mean square error of a relative frequency p is

$$\epsilon = \pm \sqrt{p(1 - p)/n}$$

If we find, for instance, that in adult foreign-born Hebrew males the cephalic index of 82 occurs 113 times among 764 individuals, or with a relative frequency of $113/764 = 0.148$, the mean square error will be

$$\epsilon_1 = \pm \frac{\sqrt{0.148 \times 0.852}}{\sqrt{n}} = \pm \frac{0.355}{\sqrt{n}}$$

Thus there would be left an error in a total series of

100 observations,	of	0.0355,	or about	24	%
900 "	"	0.0118, "	"	8	%
10,000 "	"	0.0035, "	"	2.4	%
40,000 "	"	0.0018, "	"	1.2	%
60,000 "	"	0.0015, "	"	1.0	%

For probabilities near 0.5 the result is a little more favorable, but distributions consist throughout of small probabilities for the value of each measurement.

These data show that an exact knowledge of the distribution of frequencies is practically impossible. If, however, a distribution exists which is characteristic of a certain type, then it is clear that each value which depends in a definite manner upon the distribution has a constant value. Since there is no reason for considering one observation as more valuable than another, it seems convenient to determine a value into which all observations enter in the same manner. Such a value is the average, or arithmetical mean. According to what has been said, the arithmetical mean has only one value for each type of distribution, so that it is impossible that

two series should be identical that have different averages. If the mean square variability σ of the series of n observations is determined, then the mean square error of the average is $\epsilon = \pm \sigma/\sqrt{n}$.

In the series of cephalic indices of adult foreign-born Hebrew males we find the average

$$a = \quad 83.0$$
$$\sigma = \pm \quad 3.2$$
$$n = \quad 764$$

The error of a is therefore

$$\epsilon = \pm 3.2/\sqrt{764} = \pm 3.2/28 = \pm 0.11$$

According to the theory of probabilities (as has been stated on p. 537) we may bet in the following way that the true average is between the limits mentioned:

68	against	32	that it is between 82.89 and 83.11
95	"	5	" " " " 82.78 " 83.22
99.5	"	0.5	" " " " 82.67 " 83.33

It is therefore quite easy to mark the limits which determine that the difference between two averages is significant. If the difference between two averages is significant, the two series cannot possibly be the same. The average is therefore the first criterion for distinguishing two series. This is a purely arithmetical procedure and has nothing whatever to do with the interpretation of the average, which is not involved in the process.

Mr Radosavljevich's opinion that the change of an average does not imply a change of type is therefore fundamentally wrong. The reverse is the basis of all anthropometrical work.

Besides the average, the mean square variation σ fulfils the condition mentioned before, namely, that all observations are considered in the same manner. This value has been defined before (p. 537). The accuracy of this measure is determined by the error

$$\pm \sigma/\sqrt{2n}$$

The theory of probabilities shows that in series which are dependent on many accidental causes only, the two values a and σ, i. e., average and variability, give us the theoretical distribution of an infinitely long series. For instance, the value of

$$a = 83.6 \qquad \sigma = \pm 2.8 \qquad n = 895$$

for the cephalic index of foreign-born Hebrew women (Partial Report, p. 82) gives us the following theoretical and observed frequencies (in per cent) of indices under 80, between 80 and 85, and over 85.

	OBSERVED	THEORETICAL	ERROR
< 80	6.7%	7.2%	± 0.8
80–85	55.5%	56.1%	± 1.7
> 85	38.3%	37.2%	± 1.6

For the cephalic index of foreign-born Hebrew men (Partial Report, p. 72) we find

	OBSERVED	THEORETICAL	ERROR
< 80	12.8%	13.6%	± 1.2
80–85	57.8%	55.4%	± 1.8
> 85	30.4%	31.0%	± 1.6

It will be noticed that these deviations are quite within the limits of errors that may be expected. The average and variability are thus seen to be not only in most cases an adequate expression of the whole distribution, but that they also give at a single glance a clearer impression of the character of the series than does the inconvenient tabular statement of the observed frequencies. It is entirely unjustifiable, as has already been stated, to assume that part of the series which lies under and over the arbitrary points 80 and 85 to represent distinct types.

I will discuss at this place the objection raised by Mr Hans Fehlinger[1] who claims that the total number of observations on which my results are based is inadequate. This I do not admit, since the summary table on page 56 of the Final Report (Abstract, p. 28), which has been drawn up for the purpose, shows that in most cases the differences between the foreign-born and American-born series are considerably larger than their mean square errors. The mean square error of the difference between two measures that have the individual errors $\sigma/\sqrt{n_1}$ and $\sigma/\sqrt{n_2}$ is $\sigma\sqrt{1/n_1 + 1/n_2}$, and their weight—corresponding to the number of observations—therefore

$$\frac{n_1 n_2}{n_1 + n_2}$$

[1] *Politisch-Anthropologische Revue*, Nov. 1911, X, no. 8, pp. 416–418.

By using this formula, the weights in the table referred to have been obtained, and we find the following approximate errors for the differences contained in the table:

	LENGTH OF HEAD	WIDTH OF HEAD	CEPHALIC INDEX	WIDTH OF FACE
Bohemians, Poles, Hungarians, and Slovacs				
Male . . . . − 0.6 ± 0.4	− 1.8 ± 0.3	− 0.8 ± 0.2	− 1.6 ± 0.4
Female . . . . . . − 0.4 ± 0.4	− 1.4 ± 0.3	− 0.7 ± 0.2	− 1.7 ± 0.4
Hebrews				
Male . . . . + **2.2** ± 0.24	− **1.8** ± 0.20	− **2.0** ± 0.12	− 1.1 ± 0.20
Female . . . . . . + 1.9 ± 0.37	− **2.0** ± 0.31	− **2.0** ± 0.19	− *1.3* ± 0.35
Sicilians				
Male . . . . − **2.4** ± 0.43	+ 0.7 ± 0.35	+ *1.3* ± 0.21	− *1.2* ± 0.35
Female . . . . . . − **3.0** ± 0.50	+ 0.8 ± 0.42	+ *1.8* ± 0.25	− **2.0** ± 0.42
Neapolitans				
Male . . . . − 0.9 ± 0.4	+ 0.9 ± 0.35	+ **0.9** ± 0.20	− *1.2* ± 0.35
Female . . . . . . − *1.7* ± 0.55	+ 1.0 ± 0.46	+ *1.4* ± 0.28	− 0.6 ± 0.46

All the values for which the probability of the result is more than 99,997 chances out of 100,000 have been printed in heavy type, those for which the probability is less than this amount, but more than 9,986 in 10,000, have been printed in italics. All these are, therefore, practically certain. The remainder might be considered as doubtful. Thus it will be seen that the length of head of the Bohemians and width of head of the Italians are the only doubtful measures. Length of head and width of face of Neapolitans appear also as certain when the measurements for males and females are combined.

Mr Fehlinger's claim that measurements of stature and head-form—which, he says, are exceedingly variable in almost all human types—lead more easily to errors than other measurements, I fail to understand. His statement that the individuals investigated are not of pure descent, but in part are children of parents of mixed nationality, is based on a misunderstanding of my work.

On the other hand, it might perhaps have been said that a psychological cause existed in the minds of the observers, which produced one personal equation for foreign-born and another for American-born. It is well known that an expected result may influence an observation. I think, however, the study of the

personal equations disproves this assumption. Besides this, the results among various types lie in different directions; the observers did not know what to expect; in many cases the statistical information was recorded by one observer, the measurements by another; and constant changes between foreign-born and American-born occurred in practice. All these make such a psychological explanation highly improbable. Here it must be considered as particularly important that the results agree with the previous observations by Ammon in Baden and Livi in Italy, which are, therefore, corroborative evidence of the accuracy of the results.

Here I must mention also the objection made by Sergi,[1] who considers the comparisons between parents and their own children as inadmissible on account of the small number of cases. He makes a rough comparison of the various series, merely counting the number of series of children of various ages in which the difference between parents and their American-born children exceeds that between parents and their foreign-born children, and vice versa. This is an inadmissible way of making the comparison. I have given these differences for Hebrew parents and children of all combined ages on page 124, and repeat here the results with their mean square errors.

Length of head	-1.65	± 0.26
Width of head	$+1.52$	± 0.20
Cephalic index	-1.60	± 0.12
Width of face	$+2.10$	± 0.20

It appears from this that these differences are also quite certain, their value being, in the case of the cephalic index, about twelve times that of its error. The probability that the differences in the two groups are due to chance, not to a definite cause, is infinitesimally small.

The differences in cephalic index between parents and their own American-born children, born less than ten years after arrival of the mother, and of those born more than ten years after the arrival of the mother, are, according to Table 18 (Partial Report,

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[1] Il preteso mutamento nelle forme fisiche dei discendenti degl' inmigrati in America, *Rivista Italiana di Sociologia*, anno XVI, fasc. I, Jan.–Feb. 1912, pp. 16–24.

p. 49, Final Report, p. 127) and the summary table 45, 2b (Final Report, p. 124), -1.37 and -1.97 respectively. Their difference is, therefore, 0.60, with a weight, according to Table 18 just quoted, of 200. This gives an error of $\pm 3.2/\sqrt{200}$ or about ± 0.22, so that the significance of this difference is also quite probable.

Mr Radosavljevich is not at all clear in regard to the question whether the differences can be considered as significant or not. He says (p. 419): "Even the differences which Boas found between parents and their children are normal differences in degree, which may be the result of the countless errors in such delicate measurements, and other causes" (I presume by "normal" he means here accidental), and (pp. 417–418): "We believe that these" (he refers to the table of cephalic indices of individuals that have been born more or less than 10 years after the immigration of their mothers, discussed on pp. 540–1 of this paper) "and other methodological errors may be just the cause of the differences, and not the American soil and financial panics. Even by those minute deviations from the average it cannot be certainly inferred that the greater variation of the figures means that the Hebrew or Sicilian is undergoing a modification of the shape of the head on American soil." I have just given the proof that the differences cannot be explained by accident, and that inadmissible inaccuracies of measurement do not exist.

Another question raised by Mr Radosavljevich relates to the uneven distribution of types. He claims that in all anthropometrical investigations the numbers of observations must be made equal for all the groups compared. This is wrong. In every statistical phenomenon there is a natural relative frequency of classes which must be guarded if we are to attain results of value. This is true particularly during the period of growth, when according to the system of obtaining material a certain selection takes place. The ideal of measuring all the children is unattainable. In grammar schools, for instance in the fourteenth year, those who go to work and those who are already in high school are not measured. When measuring high-school pupils an entirely different group is taken, which is, on the whole, better developed than the pupils of the

grammar school. At the same time, American-born pupils are more numerous in the high school, so that the difference between the two groups of American-born and foreign-born would be accentuated by mixing the two groups. Evening clubs represent, again, a different population. By filling up gaps, therefore, exceedingly complicated elements are introduced. I mention the particular case of the 14-year-old children because it gave me considerable trouble in the initial stages of the investigation. I am even in doubt if in my series the school children and children measured at home are strictly comparable, and this was additional reason for giving up measurements in schools. By the way, Mr Radosavljevich is mistaken if he thinks that the material was collected almost entirely in schools. No girls to speak of were measured in schools, and the total number of school children measured does not exceed three thousand. The irregular distribution is, therefore, one of the concomitants of a naturally selected series.

I must mention here another misconception of Mr Radosavljevich which Professor Sergi seems to share. In speaking of the difference in distribution of head indices which he cannot explain away, Mr Radosavljevich says that differences in the distribution of headforms of American-born and foreign-born children exist, but that *"it is not known what the differences were in the parents of these two groups"* (p. 409). I have treated the possibility of such differences on pages 42–44 of my Partial Report. In order to overcome this possible objection, I have investigated on the one hand parents and their own children in order to obtain strictly homogeneous material; on the other hand I have compared immigrants of each year with American-born descendants of immigrants of the same year, a comparison which insures homogeneity of material.

As a matter of fact Mr Radosavljevich undoes the whole work of his criticism in saying, "The differences found by Boas, if they have any real meaning, may be regarded as the normal differences of separate groups, such as are frequently noticed in separate parts of the same people." My only problem is the ascertaining of the occurrence of such differences between separate parts of a people,

namely, immigrants and their American-born descendants. I presume Mr Radosavljevich's assurance that differences occur "normally" does not solve the problem of their occurrence?

3. INTERPRETATION

It may be due to the rather wide attention that has been given to my investigation by the daily press and a number of magazines, and the exaggerations that are found in these articles, that some investigators believe that I have claimed to have discovered the origin of a new American type. Here I may call to witness the critical Mr Radosavljevich, who certainly would have discovered this claim in my report if it were there. In fact, nothing can be farther from my thought, and the precise expressions in my later reports are due to my anxiety to avoid the possibility of such a misunderstanding. In the Partial Report the word "American type" does not occur. All I say is this: "The east European Hebrew, who has a very round head, becomes more long-headed; the south Italian, who in Italy has an exceedingly long head, becomes more short-headed; so that both approach a uniform type in this country, so far as the roundness of the head is concerned" (p. 7). Later, at the end of the discussion of the values of the cephalic index, I say, "The diagram shows very clearly that the two races[1] in Europe are quite distinct, but that their descendants born in America are very much alike" (p. 9). I might have added again "as far as the roundness of the head is concerned." Since, however, the diagram refers to nothing else, and the remark appears in the further elaboration of the thesis quoted before, I omitted the restatement of this restriction. Again, on page 50 of the Partial Report, I say, "The effect of these changes is the development of a greater similarity of the descendants of Sicilians and Hebrews, one to the other," a statement which is strictly correct, since only the cephalic index is referred to, and not by any means identical with the claim that both develop into one human type. I may also point out that in a popular article by Mr Hendricks, published in

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[1] The term "race" was here, as in other places, introduced by the Immigration Commission. I had used the term "type."

McClure's Magazine, the only one that I had a chance to revise, the term "American type" is carefully avoided. Furthermore, in 1909, while the investigation was in progress, I disclaimed explicitly the probability of the development of an American type.[1] I will state here once more, that all I believe that has been proved are changes in various directions and of limited extent; in how far these changes may be progressive or limited by hereditary racial form, remains to be seen (Abstract, p. 53; Final Report, p. 76).

Mr Radosavljevich's criticism of what he calls my interpretation is difficult to discuss, because it seems to me that he has failed to grasp the meaning of older researches which he thought it worth while to recapitulate in connection with our problem, while he has certainly not understood my report. He constantly confuses two entirely distinct problems: the physical characteristics of the immigrants who arrive in America (for instance on pp. 394, 395), and the relation between the bodily form of foreign-born immigrants and their American-born children. As stated before, the former question has a bearing upon the latter, but has been treated only in so far as it had to be eliminated as a possible source of error. I say (Partial Report, p. 30): "The important problem of the selection which takes place during the period of immigration, and which is indicated by the change of type of immigrants after the panics of 1893 and 1907 . . . has not been studied." Nevertheless, Mr Radosavljevich thinks this is the fundamental problem (p. 395).[2]

I fear that his summary of theories in regard to changes of the form of the head does not throw much light on the question. He distinguishes:

1. A mechanical-functional theory, according to which such elements as the use of the temporal muscles, premature synostosis of sutures, etc., influence the form of the skull.

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[1] *Science*, n. s., vol. XXIX, no. 752, May 28, 1909, p. 843.

[2] At this place he calls attention to the fact that, according to my tables, after 1893 stature, length and width of head, and width of face—that is, all the absolute measurements—decreased; while the cephalic index—a ratio—increased. This he considers a contradiction of my statement; but on the same page Mr Radosavljevich quotes me correctly as saying that all the absolute measurements decreased. So what has the cephalic index to do with this question? Or does he consider it an absolute measurement?

2. The hereditary theory in the following three forms:

(*a*) The form of the head of each race has remained constant since very remote periods. In his imposing array of authorities Mr Radosavljevich has forgotten the most pronounced advocate of this theory, J. Kollmann.

(*b*) The shape of the head is inherited, but it does not assume its final shape until after birth, and it does not depend on the mixed parental value of the cephalic index.

Here two entirely disconnected points are joined, the former of which has nothing whatever to do with the question of heredity. I fail to see what the literature quoted has to do with the "theory" as here pronounced. The quotation of Ranke as disproving my own investigations on heredity does not relate at all to the point at issue,—namely, the question of mid-parental versus alternating inheritance,—since Ranke does not touch upon the similarity between parents and children at all, the question which I treated. On the other hand, he does not mention Pearson's criticism of my paper,[1] nor the whole literature on Galtonian inheritance, Mendel-ism, and alternating inheritance, which belongs here.

(*c*) The third "form of the hereditary theory claims that the *shape of the head* (or rather skull) *is inherited*, but heredity means not always absolute stability."

This, again, has nothing to do with the case, since it is merely a statement of the phenomenon of variability. Hrdlička, the only author he quotes, gives in the passage cited merely a somewhat lengthy statement pointing out the well-known fact that each individual has his own particular characteristics—hardly a theory of heredity.

3. The geographical-local theory.

Here he confuses again two entirely distinct phenomena: the phenomenon of local types which may be due to permanence of racial traits, and the unifying effects of environment.

In the whole enumeration the real questions at issue are almost entirely overlooked. These are based chiefly on the questions of (1) Hereditary influences, which include (*a*) the transmittal of acquired

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[1] *Zeitschrift für Morphologie und Anthropologie*, 1904, VII, pp. 524–542.

characters, (*b*) Mendelism, versus mid-parental and alternating inheritance, (*c*) the stability of characters and the origin of sports; (2) Selection; (3) Environmental influences, such as climate, altitude, and other geographical features, and social environment. An analysis of the phenomena of the last class must always lead ultimately to functional changes which determine the observed modifications of form.

Against the characterization of "Boas' theory as environmental-economic" (p. 405), I protest as based on a hopeless muddle of two distinct problems that have no relation whatever, namely, that of selection of immigrants according to economic conditions, and that of the changes in bodily form of the descendants of immigrants (see also above, pp. 535, 551).

To this confusion may also be attributed the criticism that my method of collecting data was not "*individualistic* but *collective* ('generalized,' *en masse*) in nature. This means that Boas did not study the effect of 'American soil' and 'financial panics' on the *same* individuals during a period of time representing the age of his subject (4 to 20 years 'and over') but he collected this data in a very short period, measuring a large number of immigrants" (p. 420). I should like to know how the effect of selection of immigrants in Europe can be studied individually; or how we are to trace the individual development of an American-born child in comparison with the same individual as a foreign-born child, for unless that can be done the method of following up the growth of each child does not help us to overcome the suspicion that there may be a different composition of the two series; and why quote all the observations on page 407 as significant if the generalizing method is not applicable? Mr Radosavljevich should at least be consistent and discard practically the whole anthropometry of growth, except some of my own and Dr Wissler's work. In the present case the individualistic method consists of a direct comparison of parents and their own children, and this I have used to the fullest extent.

I believe there is no need of occupying more space with a refutation of Mr Radosavljevich's criticisms. I shall be glad if "the

unexpected scientific results should be sifted by those who at least know the immense difficulties in attacking such complicated problems" (p. 405). Mr Radosavljevich does not know them.

I turn to the question of the interpretation of my observations and wish to repeat, first of all, my own conclusions. Starting from the observation that changes in the values of the averages occur at all ages, that these are found among individuals born almost immediately after the arrival of their mothers, and that they increase with the length of time elapsed between the arrival of the mother and the birth of the child, I have tried to investigate various causes that might bring about such a phenomenon. I have, as I believe, disproved the possibility that the difference between the two groups of American-born and foreign-born may be due to differences in their ancestry. This objection has been raised by Professor Sergi.[1] As mentioned before, the comparison of parents and their own children, and the comparison between immigrants who came to America in one particular year and the descendants who came to America in the same year, seem to me to eliminate entirely this source of error, which has been considered by me in detail.

Less satisfactory is the attempted proof of the theory that the cradling of infants has no influence upon their headform. The fact remains that among the Hebrews there is a radical difference in the bedding and swathing of infants born abroad and of those born here. Against this fact may be adduced the other one that no such radical difference in the treatment of children exists among the Sicilians, and that, nevertheless, changes occur and that these are in a direction opposite to those observed among the Hebrews. Even more unfavorable to this theory are the changes in width of face among Bohemians which develop among immigrating children who are no longer subject to such mechanical influences. I consider a further investigation into the influences of the method of bedding children desirable.

It also occurred to me that illegitimate births of children whose fathers were Americans might bring about changes. I have

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[1] Loc. cit., largely reprinted by Radosavljevich in *Science*, May 24, 1912, pp. 821–824.

disproved this assumption by proving that the degree of similarity between American-born children and their reputed fathers is as great as that between foreign-born children and their fathers (Abstract, p. 51; Final Report, pp. 154 et seq.). Besides this the social conditions of the Hebrew, Italian, and Bohemian colonies are not at all favorable to such an assumption. This point has been raised again by an anonymous English critic,[1] without, however, referring to my discussion of the question and the answer given by me.

After disposing of these points which would give the phenomenon an accidental character, without deep biological significance, I have taken up the biological problem itself, and first of all have called attention to the parallel observations by Ammon and Livi and suggested that the changes observed by them as occurring between urban and rural populations may be due to the same causes as those observed in the descendants of immigrants. If this be true, then Ammon's interpretation of the phenomenon as due to selection, and Livi's as due to the more varied descent of urban populations which makes them deviate from excessive values to more median values, must be revised.

I have also referred to the possibility that the breaking of the more or less inbred lines of small European villages after arrival of the people in America and the consequent change in the line of descent may be a cause producing changes in type.

Finally, I have pointed out that the changes can be accounted for by a process of selection only, if an excessively complicated adjustment of cause and effect in regard to the correlation of mortality and bodily form were assumed—so intricate that the theory would become improbable on account of its complexity.

It will, therefore, be seen that my position is that I find myself unable to give an explanation of the phenomena, and that all I try to do is to prove that certain explanations are impossible. I think this position is not surprising, since what happens here happens in every purely statistical investigation. The resultant figures are merely descriptions of facts which in most cases cannot be discovered

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[1] *Edinburgh Review*, CCXV, p. 374, Jan.–Apr. 1912.

by any other means. These observations, however, merely set us a biological problem that can be solved only by biological methods. No statistics alone will tell us what may be the disturbing elements in intra-uterine or later growth that results in changes of form. It may be that new statistical investigations in other types of environment may give us a grouping of these phenomena which suggests certain groups of causes, clues that can then be followed up by biological methods,—it is certainly asking too much to expect the solution of this problem from *one* series of observations. I at least am more inclined to ask for further material from other sources than to force a solution that must be speculative.

This defines my position toward the criticisms of Gaston Backman[1] and Giuseppe Sergi. The former claims that the explanations given by Ammon are adequate, and simply identifies my observations and his. He overlooks the all-important difference that I have compared parents and their own children, a method which introduces an entirely new point of view and practically disproves Ammon's claim that these changes are due to natural selection. I should like to say here that I have always considered Livi's theory as the most plausible explanation of the European observations, and still think that it must be a strong contributory cause, although it is not applicable to our series and for this reason can no longer be considered as explaining the whole phenomenon. Backman's views are, it seems, not in accord with the results of our inquiry. He states: "The causes underlying the alteration will then have to be sought in factors of selection that may be of the most divergent nature. When, nevertheless, Boas wants to maintain that he by his researches has proved the plasticity of human races, this conclusion seems to me to carry further than the facts in question will permit. It seems, on the contrary, to me to be quite plain that it is the change from country life to city life that has been the fact of real importance in the matter of the alterations which the descendants of the immigrants have undergone, and not the special American conditions. The point of weight must be sought in those conditions which the changes from country life to city life carry with

[1] *Ymer*, 1911, pp. 184–186.

them." I have shown that selection is extremely unlikely to bring about the results observed. That the essential causes may be the city conditions is possible, but not proven. I have not ventured to claim that I have discovered these causes. Besides, what would it help us if we assign the phenomena to city life, since the manner of its influence is as· obscure as that of any other cause? I may quote here from my "Abstract" (p. 52), which Mr Backman reviews (also Final Report, p. 75). When speaking of the differences between urban and rural types, noted by Ammon and Livi, I say: "Our American observations show that there is also a direct influence at work" (in so far as the differences occur also between parents and their own children, in which case selection is highly improbable and mixture excluded). "Ammon's observations are in accord with those on our American city-born central Europeans; Livi's, with those on our American city-born Sicilians and Neapolitans. Parallel observations made in rural districts and in various climates in America, and others made in Europe, may solve the problem whether the changes that we have observed here are only those due to the change from rural life to urban life. From this point of view the slight changes among the Scotch are also most easily intelligible because among them there is no marked transition from one mode of life to another, most of those measured having been city-dwellers and skilled tradesmen in Scotland, and continuing the same life and occupations here."

As long, then, as we do not know the causes of the observed changes, we must speak of a plasticity (as opposed to permanence) of types, including in the term changes brought about by any cause whatever—by selection, by changes of prenatal or postnatal growth, or by changes in the hereditary constitution of the individual. It is quite arbitrary to restrict plasticity to the last-named cause, as Mr Backman seems to do. In order to avoid this impression I have used expressly the term "instability or plasticity of types" (Abstract, p. 53).

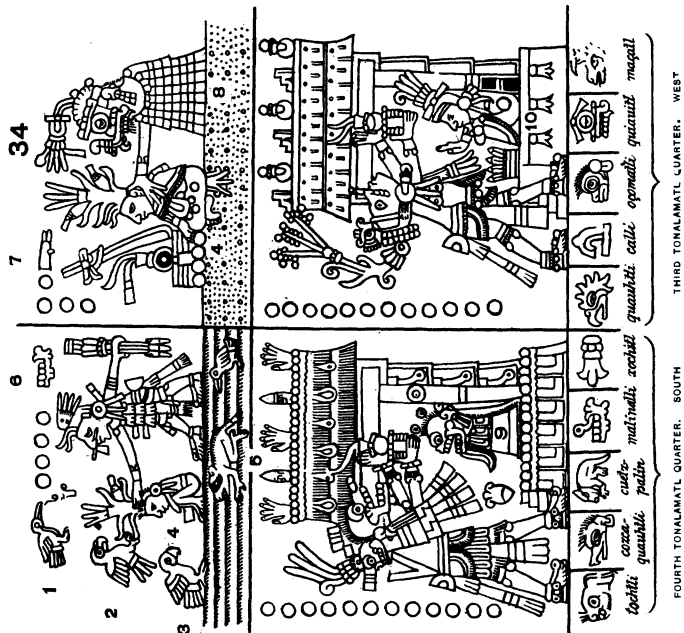Prof. R. S. Steinmetz[1] suggests that the observed changes may

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[1] Het nieuwe Menschenras in Amerika, *Nederl. Tijdschrift voor Geneeskunde*, 1911, pp. 342–352.

be due to the elimination of degenerate types that develop under the unfavorable European conditions and are, therefore, a reversion to the better developed old types. I do not consider this likely, because the conditions under which the immigrants live are not favorable; but this suggestion is worth following up as one of the possible contributory causes.

In personal conversation it has also been suggested that the lowering of the head index might be due to the increase in stature which occurs in America. I have myself pointed out that the cephalic index tends to decrease with increasing stature, because the correlation between all antero-posterior measurements,—in this case length of head and stature,—is closer than the correlation between these and transversal measurements. This relation, however, occurs only in a group which has been treated as a statistical unit. As soon as the groups are classified from distinct social or racial points of view, it ceases. This question has been treated by E. Tschepourkowsky.[1] It is clear that the same relation cannot be expected between stature and head measurements in a group which contains individuals of only one selected stature, as in a group in which all statures are increased owing to some cause that affects the whole group, and which may affect other measurements in peculiar ways.

Professor Sergi criticizes my views from the standpoint that he considers sudden changes in germplasm in new surroundings impossible and tries to reduce the phenomenon entirely to one of varying composition of the series, that is, if we follow his ideas out, to a differing fertility or mortality of component types of the immigrants. If his remarks, as it may seem, should indicate that he considers brachycephalic, mesocephalic, and dolichocephalic individuals as distinct types, the criticisms made before hold for his view also. His is an attempt to explain the phenomena by natural selection, the success of which, as said before, I consider as extremely doubtful. The particular form in which it is presented by Professor Sergi is based on his method of analyzing the somatological types constituting a people. I cannot consider his method as fruitful, since

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[1] *Biometrika*, IV, pp. 286–312.

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FIRST TONALAMATL QUARTER. EAST

ecatl itzcuintli tecpatl maquiztli ozelotl atl olin cozatl acatl cipactli

SECOND TONALAMATL QUARTER. NORTH

THIRD TONALAMATL QUARTER. WEST

guauhtli calli ozomatli quiauitl maçatl

FOURTH TONALAMATL QUARTER. SOUTH

tochtli cozca- cuetz- malinalli xochitl
guauhtli palin

DEITIES OF THE FOUR QUARTERS.

FROM THE CODEX FEJÉRVÁRY-MAYER

1. Ilhuicatl, Heaven. 2. Ecatl, Wind. 3. Toctli, Maize plant. 4. Tlatoyaualiztli, Outpouring of Water. 5. Chalchiuhtli icue, Water Goddess. 6. Olin, Motion. 7. Tezcatlipoca itztlacolihqui, God of Stone, of Punishment. 8. Tonatiuh, Sun God.

1. Tsanatl, Jackdaw. 2. Alo, Arara. 3. Cocho, Parrot. 4. Toctli, Maize plant. 5. Quimichin, Mouse. 6. Malinalli, Twisted. 7. Maçatl, Deer. 8. Tlaloc, Rain God. 9. Mictlantecutli, Lord of Hades. 10. Cinteotl, Maize God.

the analysis which he demands is impossible. If we establish a number of arbitrary types it is always possible to analyze a series of observations accordingly, but this analysis does not prove the correctness of our subjective classification and the existence of the selected forms as types, but is due merely to the fact that the distribution of observations *can* be made according to any fitting theory; but the correctness or incorrectness of the theory can be proved only in exceptional cases. I will give a definite example:

I can assume that the east European Hebrews, who have a variability of ± 3.2, consist of three elements, which have the characteristics that one element has a cephalic index ranging about 86, another one an index ranging about 83, and a third one an index ranging about 80. If, furthermore, I demand for each an equal variability, and assume that each series as well as the averages of the first four powers of the observed series follow the exponential law, the following composition results:

Variability of each series ± 2.7.

Number of cases of the series ranging about the index 83 four times the number of cases of series ranging about the indices 80 and 86 respectively.

The theoretical distributions for the homogeneous and compound series, and the observed series, for 764 cases, are given in the accompanying table. It will be seen that both series are so much alike that the one represents the observed series not appreciably better than the other.

If we analyze several series of this kind whose averages differ, making the assumption that they are composed of three equidistant elements of equal variability, the observed result can be obtained only by unequal frequencies of the constituting elements. If we call the variability of the observed series v, that of the component series σ, the distances between the averages of the component elements d, that between the observed average of the whole series and the middle component group d_0, and the number of occurrences of the series with smallest average n_1, of that with the middle

| | THEORETICAL VALUES | | OBSERVED VALUES |
| | *Homogeneous* | *Compound* | |
Cephalic Index	*Series*	*Series*	
71	—	—	3
72	—	—	—
73	1	1	—
74	2	1	1
75	4	4	2
76	9	8	9
77	17	16	13
78	28	26	28
79	45	44	42
80	61	61	57
81	77	80	77
82	89	94	113
83	98	95	99
84	89	94	76
85	77	80	77
86	61	61	67
87	45	44	41
88	28	26	21
89	17	16	22
90	9	8	7
91	4	4	7
92	2	1	1
93	1	1	1

average 1, of that with the highest average n_2, we can show, under the assumptions made before, that

$$n_1 + 1 + n_1 = 3(d^2 - d_0^2)/2(d^2 + d_0^2),$$
$$n_1 = \tfrac{1}{2}d\{(n_1 + n_1)(d - d_0) - d_0\},$$
$$v^2 - \sigma^2 = \tfrac{1}{3}(d^2 - 5d_0^2).$$

According to these formulas the composition of the series would change very rapidly with small changes of the average. For instance:

598 Bohemian foreign-born females have the average index 84.6 ± 3.2
211 Bohemian American-born females have the average index 84.3 ± 3.3

If the constituent elements of these series have the averages 81, 84, and 87, then the constituent elements would occur with the following frequencies per hundred:

SERIES	81	84	87	σ
Foreign-born	3	74	23	± 2.8
American-born	11	68	21	± 2.7

Such rapid changes in the composition of the series, due to very slight and therefore uncertain changes of the averages and the consequent asymmetries, do not seem at all plausible. It may also be pointed out that in the case here discussed the difference of the average must not be more than ± 0.72, in order to make the analysis possible.

In other words, if the theory of a compound origin of the series is to be maintained without other evidence than that contained in the distribution of observations in the series, it must be proved that the shifting of the average is associated with certain types of skewness. It may also be pointed out that in most cases in which the series can be proved to be compound, disturbances of the correlations between various measures will be found[1] that may corroborate or disprove the theory. In our series there are indications neither of significant asymmetries nor of disturbances of correlations.

It follows from all this that it is inadmissible to attempt an analysis on an arbitrary basis, unless proof can be given that the supposed constituent elements have biologically separate origin. The greater the number of types that are to be segregated, the more arbitrary becomes the method, and almost any analysis according to a sufficient number of types *can* be made. There are, of course, distributions that demand an analysis—like von Luschan's bi-modal curves of Asia Minor, or my own for width of face of half-blood Indians, and others,—but there must be strong internal evidence of a compound character, and even then the analysis will be arbitrary if the component types are not known. This is perfectly evident if we realize that each type must be defined by at least three constants,—average, variability, and relative frequency,—so that for two component elements five constants must be determined (one value of the relative frequencies being determined by the relative

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[1] See F. Boas, The Cephalic Index, *American Anthropologist*, n. s., vol. I, 1899, pp. 448–461.

frequency of the remaining series), for three elements eight, etc. The greater the number of constants to be determined, the better can the theoretical and observed series be made to coincide, almost regardless of the correctness of the theory which is expressed by the constants.

I conclude from this that the claim that the change must be explained by a different composition of the series of American-born is inadmissible, because it is an entirely arbitrary solution of the problem.

I repeat that I have no solution to offer, that I have only stated the results of my observations and considered the plausibilities of various explanations that suggest themselves, none of which were found satisfactory. Let us await further evidence before committing ourselves to theories that cannot be proven.

Finally, a few words on the opinion that has been expressed or implied, that our observations destroy the whole value of anthropometry, in particular that the study of the cephalic index has been shown to have no importance. It seems to me, on the contrary, that our investigations, like many other previous ones, have merely demonstrated that results of great value can be obtained by anthropometrical studies, and that the anthropometric method is a most important means of elucidating the early history of mankind and the effect of social and geographical environment upon man. The problem presented by the geographical distribution of headforms,—for instance, of the cephalic index,—has not been solved by our inquiry. All we have shown is that headforms may undergo certain changes in course of time, without change of descent. It seems to my mind that every result obtained by the use of anthropometric methods should strengthen our confidence in the possibility of putting them to good use for the advancement of anthropological science.

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